

as the budget of NIH grew. Also, by the end of January, drug experiments had started in the Pigeon Lab, made possible by Ferster, and before long I was making drug solutions in the medical school for other people in the Pigeon Lab, notably Morse and Herrnstein. Naturally, I wanted to do experiments in my own lab at the medical school, but not, of course, on pigeons. But Ferster offered me a complete setup for pigeons to take over. The temptation was too great, so pigeons arrived in the medical school, to nobody's subsequent regret.

The people in the Pigeon Lab were hard working and enthusiastic and knew they were in an excellent lab. But I do not think they fully grasped what epoch-making discoveries they were making; perhaps not even Skinner

realized, as judged by his writings during the Pigeon Lab era compared to his pre-1950 writings. It is well known that *Schedules of Reinforcement* (1957) met with a cool reception. Skinner had spent little time cultivating the favor of his psychological bretheren; indeed, he was openly contemptuous of much in contemporary psychology. But, then, it also took time for Gregor Mendel to be appreciated.

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Edmund Fantino (1961–1964)

THE NURTURING OF A BEHAVIOR ANALYST

The Pigeon Lab is best understood in terms of the intellectual context in which it was embedded. The Department of Psychology at Harvard, at least in the early 1960s, was partitioned into three enterprises: the newly minted Cognitive Center, spearheaded by George Miller, a founder of the "information-processing revolution"; the Psychophysics Laboratory, directed by S. S. Stevens, developer of the psychophysical law; and the Pigeon Lab. When I arrived, fresh from a BA in mathematics, I was deciding between psychophysics and cognition. I had little interest in behavior analysis, regarding it as too restrictive and narrow to deal with the rich tapestry of human behavior. Two and a half years later I turned in a doctoral dissertation on risky choice in the pigeon and was a committed behaviorist. I believe my transformation was a function of the structure of the department's graduate program and the nature of the scientists working in the Pigeon Lab at the time. These influences, the basis of my commentary, undoubtedly affected others as well. For throughout the 1960s, a large proportion of the bright, undecided, incoming students gravitated towards the Pigeon Lab.

A critical aspect of the graduate program was its emphasis on breadth. All 1st-year students were required to conduct a research project in each of the three areas. We did this in groups of three so that three reasonably substantial projects could be completed in one academic year. For the pigeon project, Bill Krossner, Joyce Shaw, and I (directed by George Reynolds) studied sound localization. This project involved a greater degree of methodological complexity than those we did in the other laboratories, and the individual data seemed more meaningful than those from the other projects. Suddenly I grasped the possibility that behavioral techniques offered solutions for problems of broad general interest. My career choice had been complicated. Breadth was also insured by the department's preliminary exams. This was a set of four 3-hr exams, offered on consecutive days, before the start of classes in the fall semester. Typically one had to grapple with sensation and perception; learning and motivation; thought and cognition; and physiological psychology. Preparing for these four exams was arduous, and in all cases involved an appreciation of the field's historical un-

derpinnings as well as current developments. The learning and motivation exam was not simply a review of operant conditioning. Traditional learning theories were emphasized as well as developments in, say, mathematical learning theory. In 1963 I don't recall a single question on behavior analysis, an indication that even the Pigeon Lab academics were emphasizing breadth, assuming that we would pick up knowledge of conditioning in seminars and in research. This breadth put us in good shape for subsequent teaching positions. (In my 1st year at Yale, I was required to teach sensory processes, perception, motivation and statistics; thanks to the prelims and to an exam requirement in probability and measurement, I was able to do so without embarrassment. In fact, when I asked to teach a graduate seminar in learning the next year I was turned down and was offered a graduate seminar in perception instead—one of my fondest teaching experiences.)

The breadth of the program in experimental psychology not only permitted students to gain a strong background in the discipline but also forced them to sample the strengths and weaknesses of the approaches offered. It seems that in those days the Pigeon Lab came out best. One reason was the presence of dedicated young researchers who were around at all hours creating an intellectually exciting atmosphere. Although we all profited from interactions with Skinner and Richard Herrnstein, in 1961–1962 the constant presence of recent Pigeon Lab PhDs George Reynolds and Charlie Catania helped promote interest in the operant approach. Of the seven entering students in the fall of 1961, four of the six who continued chose to emphasize research in the Pigeon Lab (Lois Hammer, Bill Krossner, John Staddon, and me). Although Catania and Reynolds left in the summer of 1962, the next batch of incoming students had this committed group of 2nd-year students to provide enthusiasm for the Pigeon Lab. One of those incoming students (Howie Rachlin) told me that was a major reason why he chose the Pigeon Lab. Interactions among students were fostered by the “living arrangement” of six students sharing an office.

The department's seminars were scheduled in the evenings, and the arguments and discussions that occurred there often continued at the watering holes of Cambridge until clos-

ing. What wasn't available at Harvard often was at MIT. Thus, several of us took brain and behavior seminars with Hans Lucas-Teuber there. In addition, Allen Neuringer, Richard Schuster, and I took a physiological psychology laboratory with Steve Chorover at MIT that occupied our Thursday afternoons and part of our evenings (after which Schuster returned to his family and Neuringer and I repaired to Jenny's in the North End where we would review the day's activities over pasta and an authoritative house red).

But the seminar that most influenced me came in my 2nd year. One of the articles in this learning seminar, taught by Jim Holland and Brendan Maher, was Chomsky's (1959) infamous review of Skinner's *Verbal Behavior* (1957). Skinner had assigned much of *Verbal Behavior* in a seminar the prior year. Although I found the book interesting, I felt that it fell short of an adequate explanation. My reservations about the potential applicability of behavior-analytic principles to human behavior had not been assuaged by my reading of the book. Reading Chomsky's review changed everything. After getting past his inappropriate diatribes against drive-reduction notions, I began examining Chomsky's claims regarding the inadequacy of Skinner's account. I found myself defending the approach, often after going back to the text and reviewing a relevant portion. By the time I was through I had come to the conclusion that *Verbal Behavior* was a grand achievement and that behavior analysis had the potential for answering the largest questions about human behavior, a position I hold to this day (e.g., Fantino, 1998a, 1998b; Stolarz-Fantino & Fantino, 1990, 1995). I have never looked back.

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REMINISCENCES OF A REFORMED PIGEON PUSHER

It is impossible for me to separate my work as a research assistant in the Harvard Pigeon Lab from my experiences with Fred Skinner. Before I joined the lab, I was told by one of his former assistants at Indiana University what I should expect. He warned me that I would be given responsibility to find ways to achieve a particular behavioral objective by whatever mechanical, electric, or other means I could find, and that it would be up to me to find ways to succeed at it. One had to grasp whatever Fred had in mind by way of a research objective and then find a way to achieve it. The emphasis was on initiative and originality. No one could complain that his assistants worked within a straitjacket. The atmosphere of freedom of inquiry in which we all worked in that environment was the salient feature of those years for me. As a 2nd-year graduate student, I completed two studies using rats and pigeons before settling on my thesis problem. Both of these were subsequently published, and when I apologized that my acknowledgment for support from the lab had been lost somewhere in the shuffle, Fred replied reassuringly, "We don't exact tribute here." Independence of effort was not only encouraged; it was expected.

The program of the Pigeon Lab gave experimental psychology its flagship research in the field of learning. Not apparent at the time to those of us preoccupied with the effects of schedules of reinforcement, species-specific behavior, differential reinforcement of low and high rates, and rigging ping-pong demonstrations was the subtle influence of Skinner's concept of the operant, which implicitly defined what a true science of human behavior must eventually become. Although the research program shifted focus several times from studies of the effects of schedules of reinforcement to implications of aversive control, the fine structure of visual discrimination, drug effects, and the like, the pre-

vailing theme was that of inductive pragmatism. Regardless of the occasionally impressive swirls of theoretical obfuscation that typified those times, we all knew that what we were doing "worked." That, plus Skinner's oft-repeated observation, "The subject is always right," kept us close to the language of the data. Fred Skinner was impatient with abstract philosophical arguments mainly because he understood their implications so clearly. I recall how the objection by some of our colleagues that Gödel's proof challenged the validity of empirical research was dismissed with a snort to the effect that, like it or not, the experimental approach worked, and so it did. On another occasion, Skinner expressed irritation that another colleague had once made the point that despite his protestations, he did indeed have a theory. His reply was that if thinking the sun is going to rise tomorrow because it always has is a theory, then he guessed he did have one. In any case, he was never opposed to theory as such, only to bad theories.

In the years following, I have often found myself describing the work of the lab to my own students as a place where anything could find its place into the body of science, no matter how unexpected. There was no overriding preconception that ruled where research should or should not go. All that new facts needed for admission to scientific respectability was that they meet minimal operational requirements. New concepts had to be publicly replicable to be verified and accepted.

Programmatic research of the kind pursued in the Pigeon Lab is now rare or impossible for many reasons. One is the "flight from the lab" that Skinner himself decried. Psychology is a field entranced with pop culture and quick fixes. Cognitive science, when it is not resurrected structuralism or committed to proving that computers think just like